Translating experimental paradigms into individual-differences research: Contributions, challenges, and practical recommendations

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Abstract

Psychological science has long been cleaved by a fundamental divide between researchers who experimentally manipulate variables and those who measure existing individual-differences. Increasingly, however, researchers are appreciating the value of integrating these approaches. Here, we used visual attention research as a case-in-point for how this gap can be bridged. Traditionally, researchers have predominately adopted experimental approaches to investigating visual attention. Increasingly, however, researchers are integrating individual-differences approaches with experimental approaches to answer novel and innovative research questions. However, individual differences research challenges some of the core assumptions and practices of experimental research. The purpose of this review, therefore, is to provide a timely summary and discussion of the key issues. While these are contextualised in the field of visual attention, the discussion of these issues has implications for psychological research more broadly. In doing so, we provide eight practical recommendations for proposed solutions and novel avenues for research moving forward.

Keywords: attention; visual attention; individual differences; correlational; experimental; reliability; variability; methodology.

Psychological science has long been cleaved by a fundamental divide between researchers who experimentally manipulate variables, typically in the laboratory, and those who examine existing individual-difference variables, often via questionnaires. Increasingly, however, researchers are appreciating the value of integrating these approaches (Hedge, Powell, & Sumner, 2018). However, this integration is not straightforward, since many of the research practices that benefit one of these approaches compromises the other. Here we use the field of visual attention research as a focal example for highlighting the challenges that arise, and making practical recommendations for solutions. This provides an excellent case study since it is a field that has historically been firmly in the experimental camp, but within it there is a growing zeitgeist with respect to adopting an individual differences framework. While the examples used here are contextualised in the field of visual attention, it is important to emphasise that the issues discussed and recommendations apply for psychological research more broadly.

Attention is a psychological concept that is readily invoked by scientists and non-scientists alike. Imperatives such as “pay attention” are a part of everyday parlance, because people appreciate that the application of attention, or the failure to do so, has real consequences, including potentially enhanced processing of attended information, and the de-prioritised processing or even complete ‘missing’ of stimuli that are not attended. In the scientific community, while attention has been studied in multiple sensory modalities, perhaps the greatest research interest has been in the domain of visual attention. That is, there is often far too much information in visual scenes to be able to process to the level of conscious awareness. This means that visual attention plays a crucial triaging role in prioritising the most relevant or salient stimuli for processing, while filtering out other content to prevent capacity-limited resources from being overwhelmed. There are several major ways in which humans can regulate their visual-attentional resources across space: the central focus of attention can be shifted in space, due to changes in the external environment, the observer's internal goals, or an interaction of these two factors (Becker, Folk, & Remington, 2010; Chasteen, Burdzy, & Pratt, 2010; Fischer, Castel, Dodd, & Pratt, 2003; Folk, Remington, & Johnston, 1992; H. J. Muller & Rabbitt, 1989; Posner, 1980; Posner & Cohen, 1984; Posner, Snyder, & Davidson, 1980), the size of the attended area (also known as attentional breadth) can be contracted or expanded (Belopolsky, Zwaan, Theeuwes, & Kramer, 2007; C. W. Eriksen & St. James, 1986; Goodhew & Edwards, 2017; Goodhew, Lawrence, & Edwards, 2017; Goodhew, Shen, & Edwards, 2016; LaBerge, 1983; Rowe, Hirsh, & Anderson, 2007), and the focus of attention can change shape (Jefferies & Di Lollo, 2015) or split into multiple foci (M. Muller, Malinowski, Gruber, & Hillyard, 2003). The operation of these different mechanisms can each have different impacts on task performance.

The scientific study of visual attention has a long and illustrious history in cognitive psychology, and more recently cognitive neuroscience. Cognitive psychologists have been fascinated with themes such as the level of processing of task-irrelevant stimuli (Lavie, 1995, 2005; Lavie, Hirst, de Fockert, & Viding, 2004; Schreij, Owens, & Theeuwes, 2008), how humans search for targets across space (Cave & Chen, 2016; Duncan & Humphreys, 1992; Treisman & Gelade, 1980; Wolfe, 2007; Wolfe, Cave, & Franzel, 1989) and time (Dux & Marois, 2009; Kozhevnikov, Li, Wong, Obana, & Amihai, 2018; Raymond, Shapiro, & Arnell, 1995), and what types of stimuli can 'grab' our attention to locations against our will (Eimer & Kiss, 2007; Jonides & Yantis, 1988; Lipp & Waters, 2007; Pratt, Radulesco, Guo, & Abrams, 2010; Sunny & von Muhlenen, 2011; Vromen, Lipp, & Remington, 2015). These questions have resonated in cognate areas, such as experimental approaches to understanding psychopathology. For example, it is now well-documented that highly anxious individuals tend to selectively apply their attention to threatening information (Bar-Haim, Lamy, Pergamin, Bakermans-Kranenburg, & van Ijzendoorn, 2007; MacLeod, Mathews, & Tata, 1986), and that ameliorating this bias can relieve the symptoms of anxiety (MacLeod & Clarke, 2015).

Furthermore, some of the earliest studies using functional magnetic resonance imaging (fMRI) in cognitive neuroscience addressed attention-related questions, such as whether features or objects are the core unit of attentional selection (O'Craven, Downing, & Kanwisher, 1999). Of course, fMRI continues to be useful in answering fundamental questions about attention (Silk, Bellgrove, Wrafter, Mattingley, & Cunnington, 2010; Tompary, Al-Aidroos, & Turk-Browne, 2018; Weng, Lapate, Stodola, Rogers, & Davidson, 2018), as well as clinical questions that implicate attentional processes (Mickleborough et al., 2016; Salmi et al., 2018). Since the distinction between cognitive psychology and cognitive neuroscience is more artificial and a historical consequence of the preferred or most popular dependent variable, rather than the scientific question of interest, this distinction will not be maintained. Instead, here, *cognitive psychology* will be used as the broad umbrella term to incorporate all scientists interested in how cognition works – for which the most appropriate tool, whether it be behavioural or neural, can be selected to answer the relevant question about the psychological concept under study.

The dominant approach in cognitive psychology to studying human visual attention, irrespective of the specific dependent variable, is one that can be characterised as *experimental*, in contrast to an *individual-differences* (or correlational) approach. That is, scientists experimentally manipulate independent variables of interest, and measure the impact of these manipulations on dependent variables. Here, the focus is on how the sample or group as a whole, on average, responds to these manipulations. In other words, in most experimental work the focus is on within-participant variation, typically with the concomitant goal of eliminating between-participant variation. There are classic visual-attentional effects in cognitive psychology that are very robust and reliable at this group level, such as the Stroop effect, the

Flanker effect, and the Posner cueing paradigm, all of which stem from experimental manipulations (e.g., of congruent versus incongruent trials) (B. A. Eriksen & Eriksen, 1974; Hedge et al., 2018; MacLeod, 1991; Posner, 1980; Stroop, 1935). This approach has been the mainstay of cognitive psychology for decades, and while it has been an incredibly valuable approach that has garnered much insight into human cognition, it also remains true that individuals do differ. Therefore, this necessitates that a complete science of human cognition incorporate individual-differences into its theories and models.

Empirically, under the individual-differences approach, key variables that are intrinsic to individuals are measured, and the goal is usually to understand how these affect behaviour (i.e., the focus is on between-participant variation). This approach has been adopted in conjunction with experimental paradigms in the literature to answer distinct types of questions. On the one hand, it has been used to answer questions about key variables along which individuals differ, such as working memory capacity, and their impact on performance under different experimental conditions (Robison & Unsworth, 2017). For example, individuals with high working memory capacity can deploy attention in the form of an annulus under particular task demands, whereas individuals with low working memory capacity were found not to be able to under the same conditions (Bleckley, Durso, Crutchfield, Engle, & Khanna, 2003). This shows how the answer to questions regarding how flexible the allocation of visual attention is can depend on another variable – in this case, working memory capacity. Furthermore, while working memory capacity can be conceptualised as a “cognitive” variable, even more traditional affective-behavioural variables such as personality have been tested and found to affect an individual’s breadth of attention across space (Wilson, Lowe, Ruppel, Pratt, & Ferber, 2016) and the allocation of attention across time (Maclean & Arnell, 2010), as measured by the attentional blink, the deficit in identifying a second target in a rapid serial visual presentation stream when it occurs within several hundred milliseconds of the first target

(Dell'Acqua et al., 2015; Dux & Marois, 2009; Raymond, Shapiro, & Arnell, 1992). These relationships can be used both to provide insight into an attentional concept itself, or the difference in the attentional process can be used to provide insight into another psychological concept. For example, the relationship between individual differences in spatial attentional breadth and the attentional blink is consistent with the overinvestment hypothesis of the attentional blink (Dale & Arnell, 2015). On the other hand, leveraging what is known about these attentional processes, attentional tasks such as the attentional blink have been used to differentiate between distinct theories of psychopathy, revealing that psychopathy reflects an attentional bottleneck that interferes with processing contextual information (Newman & Baskin-Sommers, 2011; Wolf et al., 2012).

Moreover, the individual-differences approach has also been used to test questions about the theoretical or conceptual relationship between cognitive concepts, such as between attention and working memory (Kreitz, Furley, Memmert, & Simons, 2015), and operationalisations of concepts, such as whether different versions of the attentional blink task reflect the same underlying psychological construct (Dale, Dux, & Arnell, 2013). Where there are stable and measurable individual differences, it also offers important potential practical benefits, such as personnel selection for attention-demanding roles, and/or assessing which individuals will be most responsive to training in such contexts.

This analysis demonstrates that there is a growing appetite for individual-difference approaches to complement experimental approaches in cognitive-psychological research. The purpose of this review paper, is to provide a timely explanation and discussion of the key challenges and issues about as this approach progresses in the field. These issues emerge because there are fundamental differences between the traditional experimental and individual-difference approaches. Cognitive psychologists are typically trained and well-versed in navigating the issues of experimental research, but not always those of the individual-differences approach. However, the precise methods used to reduce between-participant variation in experimental designs can undermine individual-differences approaches. That is, the approaches are not just distinct, but methods and practices that benefit them can be *mutually incompatible*. For instance, methods that reduce between-participant variation that benefit reliability in experimental designs, undermine reliability of rank-ordering individuals in within-participant designs (Hedge et al., 2018). Furthermore, the importance of measurement reliability which is such at the forefront of standard individual-differences (e.g., questionnaire-based) research, is often overlooked in cognitive research. Moreover, some areas have been plagued by inconsistent and what appear to be unreliable findings, whereas new approaches have revealed novel frameworks for considering performance where consistency within individuals is revealed. In this next section, therefore, these key issues will be highlighted and some recommended solutions proposed.

A first and crucial step is considering the reliability of attentional tasks in individual-difference or correlational designs. Measurement reliability is important because the ability to find correlations among measures is dependent on the reliability of these measures in isolation. In other words, the maximum possible correlation is constrained by the reliability of the individual measures feeding into the correlation (Spearman, 1910). The importance of reliability is well understood in traditional questionnaire correlational research, with measures of internal reliability and test-retest reliability routinely reported and interpreted, and reliability used as a criterion for measure selection (Cicchetti, 1994; Kenny, Bizumic, & Griffiths, 2018). The importance is not as clearly at the forefront of cognitive psychologists' minds, even when an individual-differences approach to research is adopted. It is much less common to see reliability scores for experimental tasks reported in individual-differences studies, even for prominent and influential visual-attentional studies (e.g., Huttermann, Memmert, & Simons, 2014; McKone et al., 2010; Wilson et al., 2016).

For example, Hutterman et al. (2014) examined the breadth of attention in athletes. To do so, these authors used an interesting method, which involved presenting two stimuli in different parts of the screen. Each stimulus consisted of four, adjacent but non-overlapping shapes (circles and squares) that could be either light or dark grey. Participants' task was to report the number of light grey triangles (firstly in one stimulus and then the other), and they were only deemed correct if they reported the number of light grey triangles correctly for both. The distance between the stimuli along a given meridian (horizontal, vertical, diagonal) at which participants could score 75% correct was considered the breadth of their attention. Across several experiments, the authors found that, in general, participants had a wider breadth of attention along the horizontal meridian than either the vertical or diagonal, but that expert versus novice sports players in particular had a wider breadth along all meridians. There was also a sport-specific effect: players of sports for which the horizontal meridian is particularly important (e.g., soccer) showed a more pronounced benefit along the horizontal meridian, whereas players of sports for which the vertical meridian is crucial (e.g., basketball, volleyball) showed greater attentional breadth on this meridian (Huttermann et al., 2014). In their paper, Hutterman et al. (2014) explicitly considered the inter-rater reliability of the classification of sports as horizontal versus vertical, but did not explicitly discuss the reliability of their measurement of attentional breadth. One could mount a counterargument that the fact that they obtained significant group differences demonstrates individual stability and therefore mitigates this need, however, this is not the case, and in fact their later work has shown that this measure is sensitive to state-level effects (e.g., mood) (Huttermann & Memmert, 2015). To be clear, we think that the Hutterman method is clever, the study is interesting and raises many potential exciting avenues for future individual-differences research, but their study highlights a far broader trend to consider reliability for self-report or more subjective classifications, rather than for attentional processes, even though reliability is equally important for the latter.

There are some notable exceptions to this omission, where the importance of reliability of experimental tasks has been explicitly and duly considered (e.g., Dale & Arnell, 2013; Dale et al., 2013; Kane, Poole, Tuholski, & Engle, 2006; Onie & Most, 2017). We propose that researchers follow this latter approach, and report reliability. Of course, when it comes to adopting new research practices, practical considerations also need to factor into the equation. That is, it is sensible to draw the distinction between what would be the *ideal* approach to address an issue, and what represents an acceptable approximation of this ideal approach but might be more practical, provided that one is mindful of what impact the approximation has in relation to the ideal approach. When it comes to reliability, the ideal approach is to assess test-retest reliability (i.e., how reliably does the task classify individuals between testing sessions). To quantify this, a correlation is calculated between participants' scores in the first and second testing sessions, whereby a higher correlation indicates greater reliability. Take for example a study in which the Stroop effect has been measured, with a view to measuring its correlation with measures of working memory capacity. The Stroop effect arises from the Stroop task, where participants' task is to name the colour of the ink and the meaning of the word either matches (congruent, e.g., BLUE) or mismatches the colour of the ink (incongruent, e.g., BLUE). Performance in the incongruent condition is compared against the congruent condition to gauge interference (Stroop, 1935). In order to measure the test-retest reliability of the Stroop effect, a researcher would need to administer the Stroop test in one testing session and calculate the magnitude of the Stroop effect, and then administer the test again (e.g., one week later) on the same participants, calculate the magnitude of the effect again, and then measure the correlation between participants' two Stroop effects.

However, measuring test-retest reliability doubles the data-collection demands, and requires that researchers can re-access the same participants for a second testing session, which is not always feasible. Therefore, an acceptable approximation is to assess the internal reliability of a measure from a single administration. For example, one can gauge whether the first half of trials classify individuals in the same or similar way to the second half of trials. Again, a correlation can answer this question. This provides insight into the reliability of the measure. The benefit of this is that it does not require researchers to collect any additional data, but instead just to consider the data that would be already collected. One issue to be aware of with this approach is that it assesses the reliability of the measure *at that point in time*. It does not permit researchers, therefore, to draw conclusions about the reliability of the measure across longer timescales. This means that a measure could be highly reliable at a given point in time as indicated by split-half reliability due to state factors (e.g., mood) rather than trait factors (e.g., personality). However, this split-half reliability approach provides useful information about any constraint placed on correlations with other such measures. For example, if in the Stroop effect and working memory experiment, there was no correlation between the Stroop effect and measures of working memory capacity, then one would want to assess the reliability of both of these measures. If the split-half reliability of the Stroop effect from a single administration of the test was high, it would not allow researchers to conclude that if individual A had a large Stroop effect in this testing session and individual B had a small one, that they would necessarily have a large and small one respectively again in one week's time. However, if the split-half reliability of both the Stroop and the working memory measures were high, then this would indicate that the absence of the correlation was not due to poor reliability of the measures. Therefore, **Recommendation #1: individual-difference studies should report measures of reliability for all measures including experimental tasks.**

Reliability has different implications in different contexts. This means that it is crucial to consider the conflicting forms of reliability for individual-differences versus experimental designs. This was demonstrated in a recent ground-breaking paper, which highlighted that not only is reliability in experimental versus individual-difference contexts distinct, but that these forms of reliability are *mutually antagonistic*, such that increasing reliability in one domain compromises reliability in the other (Hedge et al., 2018). Reliability in an experimental context refers to the ability to replicate the effect of a manipulation, as measured by the effect of the manipulation at the group level. For example, the Stroop effect is highly reliable in the sense that the difference in performance between the congruent and incongruent conditions is robustly repeatable at the group level (Hedge et al., 2018; MacLeod, 1991; Stroop, 1935). In contrast, reliability at an individual-differences level refers to the ability for a task to consistently rank-order individuals over time. For example, if individual A shows a strong Stroop effect (i.e., large difference in performance between the congruent and incongruent conditions) and individual B shows a weak Stroop effect at first test, then the Stroop effect would have high test-retest reliability if individual A shows a strong Stroop effect and individual B a weak Stroop effect at second test. Contrastingly, if individual B showed a stronger Stroop effect than individual A at second test, then this would undermine the reliability of the Stroop effect for individual differences research. In other words, individual-differences reliability seeks the maximum correlation between individuals' scores across distinct test administrations. Of course, it is apparent why these two forms of reliability are dissociable: both of these scenarios could have a consistent Stroop effect magnitude overall (i.e., reliable at an experimental level), but have different levels of underlying individual-differences reliability.

Hedge et al. (2018), however, showed that not only are these two forms of reliability dissociable, but many of the factors that improve one form of reliability actively compromise the other. For example, difference scores are a staple of cognitive-psychological research. A difference score is where a participant's performance in one condition is subtracted from performance in another. For example, in the Stroop task, performance in the congruent condition would be subtracted from performance in the incongruent condition to yield a Stroop interference score. Similarly, in measuring attentional breadth, performance on the global trials in a Navon task (hierarchical stimuli where a global shape is made up of lots of individual local elements, e.g., a letter 'T' constructed of individual 'F's) could be subtracted from performance on the local trials in order to yield a global preference score, or to measure attentional cueing, performance on the validly-cued trials (where a spatial cue appears in the same location as a subsequent target) could be compared against performance on the invalidly-cued trials (where a spatial cue appears in a different location to the subsequent target). The use of these sorts of difference scores pervades cognitive-psychological research, at least in part because they minimise the impact of baseline individual differences when the goal is to obtain a reliable experimental effect at the group level. That is, say that individual A has longer reaction times (RTs) than individual B, and both individuals complete two different experimental conditions (e.g., validly versus invalidly cued trials). If raw RTs in the invalid condition was used as a variable, then these individual-differences in baseline RT would muddy this metric. This would be exacerbated in experiments where critical manipulations were administered between participants rather than within-participants. In contrast, such individual-differences are largely accounted for by a difference score between the valid and invalid conditions, because an individual's baseline RT is a constant across this comparison. This means that the difference score yields a relatively "pure" measure of the effect of the experimental manipulation, decoupled from baseline individual differences. The potential issues with the individual-differences reliability of difference scores have been noted previously (Caruso, 2004; Edwards, 2001). However, one of the points that Hedge et al. (2018) demonstrates is that it precisely the reduction in between-participant variance which benefits the experimental design while simultaneously harming the individual-differences design. In essence, the difference score is a double-edged sword: whether it helps or harms depends on whether an experimental or individual-differences framework is adopted. This illustrates the critical importance of psychologists adopting different mindsets as they transition between the two types of research: the practices that benefit one area harm the other.

The double-edge of the difference score was reflected in a recent study by Onie and Most (2017). These authors examined both the test-retest reliability, as well as the magnitude of the relationship with self-report measure of negative affect and participants' performance in a paradigm known as emotion-induced blindness. In emotion-induced blindness, a rapid stream of items is presented typically in the centre of the screen, and participants' task is to identify the orientation of the target: the one image that is tilted 90° to the left or right of vertical. All of the other items in the stream are neutral landscape scenes. Crucially, at a prescribed interval prior to the presentation of the target, a distractor image is presented, which can either be emotionally-evocative or neutral in nature. The key finding is that even though these distractor images are task-irrelevant, the emotional image produces a deficit in identifying the target if it appears close in time to the target, relative to the neutral distractor (Most, Chun, Widders, & Zald, 2005; Most, Smith, Cooter, Levy, & Zald, 2007; Smith, Most, Newsome, & Zald, 2006; Wang, Kennnedy, & Most, 2012). Emotion-induced blindness is usually quantified as the difference in accuracy between the emotional and neutral distractor conditions for a given interval (lag) from between the distractors and the target – that is, a difference score. Emotion-induced blindness is a robust and reliable paradigm at the experimental level – it is a phenomenon that has been replicated numerous times. Indeed, one can usually experience emotion-induced blindness when shown a visual demonstration of a single of each trial type. The logic of the difference-score approach to quantifying emotion-induced blindness is appealing: individuals vary considerably in their accuracy on rapid-presentation tasks, and so the difference score should isolate the magnitude of emotion-specific deficit, independent of any such baseline differences. However, when Onie and Most (2017) compared the test-retest reliability of the difference-score metric of emotion-induced blindness, versus the raw scores for the emotion-distractor condition at a close interval between the distractor and the target, it was the raw scores that had the higher reliability, and likely as consequence of this, had stronger relationships with self-reported negative affect than the difference-score quantification of EIB.

If raw scores have greater reliability in an individual-differences sense (i.e., rank-ordering individuals), then one might reasonably conclude that these should be used in individual-differences research instead of difference scores. However, this approach raises the possibility that the individual-differences research may conflate multiple processes. That is, for example, in the case of Onie and Most (2017), part of the stability of the raw scores, and part of the shared variance with negative affect would be due to the emotion-specific effect of the distractor, but also part of the stability of the scores and the shared variance with negative affect would also be due to the other factors, such as perceptual ability to resolve rapidly presented stimuli. This means that while the reliability and the relationships might appear stronger, these are muddied by multiple processes, of which the process of interest is only one.

As another example, consider a researcher who is interested in establishing whether interference from irrelevant semantic information – as measured by the Stroop effect – is consistent in an individual over time. To test this, the researcher administers the Stroop test in two separate testing sessions, one week apart. If the researcher gauged the magnitude of semantic interference by just considering raw RT in the incongruent condition, and found a correlation of 0.9 between the two testing sessions, then this estimate of how reliable semantic interference is would be over-inflated by the stability of individual differences in participants' generic response speed. In contrast, calculating the standard Stroop difference score would remove much of this response-speed contamination effect. While the correlation would likely now be lower from this difference-score calculation of the Stroop effect (e.g., r = 0.3), this would provide a more accurate estimate of the individual-difference stability of semantic interference. Therefore: **Recommendation #2: that individual differences in generic task**

performance are separated from individual differences in process-specific performance. Here we have discussed the logic of a difference score, because it is such a widely popular approach to accounting for individual differences in generic task performance. However, it may not be the optimal method, particularly when the correlation between the two component conditions is high. There are alternatives, such as various methods of statistically controlling for baseline levels of performance (Edwards, 2001; Lawrence, Edwards, & Goodhew, 2018; Willett, 1988). Critically, however, it is important to make this separation, or multiple psychological differences confound a given reliability or correlation score.

While psychologists have long sought to mitigate the considerable individual differences in generic task performance when conducting experimental research, this variance could be friend rather than foe. That is, from another perspective, it is exciting that there is substantive individual-difference variance, as it offers the potential for this variance to be meaningfully *explained* rather than simply partialled out. Returning to the Stroop effect, there is variance due to the experimental manipulation (congruent versus incongruent conditions), and variance due to generic RTs – how quickly a participant responds irrespective of experimental condition. An experimental psychologist would typically focus on the between-condition variance, and treat this stable individual tendency to respond a particular way as nuisance variance to be subtracted out at the difference-score stage. But this is overlooking another key source of variance – and potentially quite an important one. Indeed, one observation from our own work is how surprisingly high the correlation can be between individuals' performance (e.g., accuracy/RT) in one experimental condition versus another. We routinely observe very strong correlations across different experimental conditions that are thought to reflect distinct processes. Take, for example, an individuals' RT for the global versus local trials in a Navon task. These are thought to reflect different attentional breadths, and therefore performance in them should be distinct. However, the correlation between them can be as high as .7-.9. In other words, the traditional goal of the cognitive psychologist is to explain variance due to an introduced experimental manipulation – i.e., how people perform differently on the global versus local trials. And indeed, there is some variance due to this. But given the strong correlations between conditions, one might reasonably say that this variance is being swamped by another source of individual-differences variance – how people perform on the task. It would seem counterproductive to ignore this substantial amount of variance that is ripe for explaining. There are, of course, many factors that could lead to general RT effects, including some relatively pedestrian ones such as an individual's motivation at a given point in time. However, there are likely far more cognitively interesting factors too.

If the testing is all performed in a single session, then both 'state' and 'trait' variables could influence the stability in overall performance on cognitive tasks. For example, an individual might have sluggish RTs on one day due to sleep deprivation the previous night (state variable), or another individual may have rapid RTs and high accuracy across multiple testing days due to perceptual processing speed and higher working memory capacity (trait variable). Some literature has emerged around explaining some of these overall RT effects (Brebner & Cooper, 1974; Deary, Der, & Ford, 2001; Der & Deary, 2006; Edman, Schalling, & Levander, 1983; Lahtela, Niemi, & Kuusela, 1985; Schmitz, Daly, & Murphy, 2007; Sheppard & Vernon, 2008). However, there is a lot of variance – stable variance – here to explain. Therefore: **Recommendation #3: psychologists study what state and trait variables explain stability in task performance (e.g., accuracy/RT) across different experimental conditions.**

Moreover, another implication of this inverse relationship between experimental and individual-difference reliability is that cognitive psychologists should resist the understandable tendency to reach for the most experimentally-reliable tasks or conditions as the focus of individual-difference research. Instead, the researcher should select conditions that maximise between-participant variation. For example, consider a scenario in which the Stroop effect is measured in two different types of experimental conditions. Say that for Condition A, the magnitude of the mean Stroop effect averaged across 20 participants is 200ms, with a standard deviation of 20ms, and for Condition B, the magnitude of the Stroop effect is 100ms, with a standard deviation of 80ms. Condition B, despite its smaller mean Stroop effect, is actually better suited to the cause of individual differences research than Condition A, given that here the individuals in the sample are differing to a greater extent from one another with respect to their Stroop effect (i.e., greater between-participation variation).

In a similar vein, it can be useful to consider performance in relation to a psychometric function where the primary dependent variable is accuracy at a perceptual task. For example, in research using the attentional blink paradigm, researchers might consider using the lags where the deficit in target identification is the strongest, as this provides the strongest and most reliable group-level effects. However, it is likely that lags where the group-level effect is of intermediate strength would reveal greatest between-participant variation and therefore have the ability to reliably distinguish between individuals. Of course, this is not to say that there are *no* individual-differences in the depth of the blink, as some individuals do show a heavily reduced or absent blink here (Martens, Munneke, Smid, & Johnson, 2006; Martens & Wyble, 2010). It is just to say that the discriminant validity of the attentional-blink task will likely be *greater* at lags where the overall group level of performance is at an intermediate level. This is analogous to the situation in experimental research, where the mid-point of an individual’s psychometric function (i.e., halfway between chance-level and ceiling-level performance), is used as the most sensitive measure of their threshold. That is where variation in stimulus intensity has the greatest impact on measured performance and hence is the most sensitive stimulus for observing experimental effects. For individual-differences research, it is also likely that the mid-point of this psychometric function – at the group level – will also be the most sensitive for detecting differences between individuals. In contrast, conditions where virtually all individuals succumb to an experimental manipulation to a similar extent, or conditions where virtually no individuals do, are far less likely to be sensitive conditions for individual differences research.

In order to determine the most sensitive conditions, a researcher might test multiple different versions of a task on their given sample of participants to determine where the between-participant variability is greatest. However, this is not always going to be practical, and in most instances the selection of the appropriate experimental conditions could simply be guided by examining the variance in performance across individuals for given experimental conditions in previous research. This is particularly true the more closely the conditions of the previous research resemble those of the prospective research.

When it comes to determining which conditions are most suitable for individual-differences research, one important aspect that it is worthwhile being mindful of is the distinction between individuals' *preference* and *ability*. This is illustrated nicely in the attentional breadth literature, in particular, the two different versions of the Navon task. In the directed version of the Navon task, participants are instructed to attend to either the global or local level to identify a target that is always at that level, and the effect of incongruent information at the other level is gauged (Caparos, Linnell, Bremner, de Fockert, & Davidoff, 2013; Navon, 1977). It is assumed that there is an inverse relationship between the magnitude of interference and an individual's ability to adopt the directed level of attention. In the undirected (also known as 'divided attention') version, the target can appear at either the local or global level (equi-probable), and RT to the targets at their respective levels is gauged (Gable & Harmon-Jones, 2008, 2010; McKone et al., 2010). The directed version of the Navon task can be conceptualised as measuring a person's *ability* to adopt a particular attentional scale, whereas the undirected version of the Navon task can be conceptualised as measuring a person's *preference*, since the task does not favour one level over another and thus it is assumed that individuals will adopt their preferred scale of attention. Finally, the Kimchi and Palmer (1982) task also directly gauges preference, where participants are presented with a test set of hierarchical shapes (e.g., a square made up of individual triangles), and then to asked to indicate which of two other sets of shapes they think are most similar to it. One of the comparison sets of shapes has the same global shape as the test stimulus but different individual elements (e.g., a square made up of squares), whereas the other has the same individual elements but a different global shape (e.g., triangle made up of triangles). Which comparison shape individuals choose gauges to which level of the test shape they choose to attend (Basso, Schefft, Ris, & Dember, 1996; Dale & Arnell, 2015; Hoar & Linnell, 2013; Kimchi & Palmer, 1982).

It is conceivable that there may be greater individual variability in preference than ability. Consistent with this, the directed version of the Navon task thought to measure ability has been found to show much lower reliability than the Kimchi and Palmer (1982) which gauges preference (Dale & Arnell, 2013). Furthermore, it used to be theorised that individuals with autism had a deficit in the global breadth of attention, which in contrast appears to be the preferred 'default' for neurotypical individuals (Badcock, Whitworth, Badcock, & Lovegrove, 1990; Baumann & Kuhl, 2005; Hoar & Linnell, 2013; Navon, 1977). This neurotypical global preference is consistent with models of scene perception that espouse a broad brushstroke sweep of processing followed by more focussed, detailed, local processing (Bar, 2003; Kveraga, Boshyan, & Bar, 2007). However, the findings in relation to autism were mixed. More recent research has revealed that individuals with autism do not appear to have an impairment when the task demands *requires* a broad scope of attention, but instead they preferentially opt for a local over a global scale when given the *choice* (Koldewyn, Jiang, Weigelt, & Kanwisher, 2013; Stevenson et al., 2018). In other words, individual (or clinical-group versus neurotypical control) differences were present in preference or default tendency, but not in ability. Of course, there may also be instances where ability is diagnostic in a way that preference is not. However, more generally, the take-home message is that when considering different conditions or different versions of tasks, clarifying the distinction between preference and ability may help to isolate processes of interest that are stable and characterise different individuals. Thus, **Recommendation #4: as experimental tasks are adapted into the individual-differences sphere, researchers identify what conditions or versions of the tasks produce greatest between-participant variation and individual-level reliability.**

In relation to Recommendation #4, we also suggest that attentional research could be informed by key individual variation in other domains. For example, visual processing is broadly subserved by two major processing channels, the parvocellular and magnocellular pathways, which specialise in the resolution of spatial and temporal aspects of scenes respectively (Denison, Vu, Yacoub, Feinberg, & Silver, 2014; Derrington & Lennie, 1984; Legge, 1978; Livingstone & Hubel, 1988). The parvocellular and magnocellular pathways feed preferentially into the ventral and dorsal pathways, which can be conceptualised as the ‘what’ (form) and ‘where’ (spatial localisation / motion) pathways respectively (Mishkin, Ungerleider, & Macko, 1983). Some key clinical conditions, including dyslexia (Grinter, Maybery, & Badcock, 2010; Vidyasagar & Pammer, 2008) and schizophrenia (Butler et al., 2001; Martinez et al., 2008) have been linked to selective deficits in the magnocellular and/or dorsal stream. Yeshurun and colleagues (Yeshurun, 2004; Yeshurun & Carrasco, 1998, 1999; Yeshurun & Hein, 2011; Yeshurun & Levy, 2003; Yeshurun & Marom, 2008; Yeshurun & Rashal, 2010; Yeshurun & Sabo, 2012) have amassed evidence that a spatial shifts of attention elicits a trade-off between spatial and temporal acuity at the newly-attended location, consistent with a mechanism that enhances the parvocellular pathway and suppresses the magnocellular pathway. Given the diagnostic individual differences in general magnocellular function, it seems likely that this is an attentional process for which they may be important individual differences.

Another implication of the antagonism between experimental and individual-differences reliability, which might seem counter-intuitive at first blush, is that tasks that produce less reliable group-level results may be optimal tasks to use where the goal is to distinguish among individuals. Of course, it is also possible that a task is unreliable at the group-level because the effect is small or inconsequential, or highly context-sensitive for all individuals. But another possibility is that each individual performs reliably on a task, but considerably differently from other participants, and therefore the group-level result that is observed is highly dependent on the participants in the sample. For example, imagine that two versions (A and B) of the Stroop task is run in 10 different experiments, which are identical in all respects except the sample of participants. Condition A produces a reliable Stroop effect in only 7 out of the 10 experiments, whereas Condition B of the Stroop task produces a reliable effect in 10 out of the 10 experiments. Condition A may actually be a better candidate for individual differences research than Condition B.

A real example of this comes from a body of research that has indicated that performance on attentional and perceptual tasks can be altered by the proximity of participants' hands to the visual stimuli, called the near-hand space effect (Abrams, Davoli, Du, Knapp, & Paull, 2008; Bush & Vecera, 2014; Festman, Adam, Pratt, & Fischer, 2013; Goodhew & Clarke, 2016; Goodhew, Edwards, Ferber, & Pratt, 2015; Goodhew, Gozli, Ferber, & Pratt, 2013; Gozli, West, & Pratt, 2012; Reed, Grubb, & Steele, 2006; Thomas, 2015). However, some recent research has questioned the robustness of this effect (Andringa, Boot, Roque, & Ponnaluri, 2018). In our own lab, while we obtain the effect more often than not, we have at times observed the effect robustly and subsequently the effect to be absent, where nothing is altered in the experimental methodology apart from the participants. It is possible, therefore, that certain individuals reliably demonstrate the near-hand space effect whereas others do not. More generally, this leads us to **Recommendation #5: tasks which produce small and/or unreliable experimental effects at the group level are tested for their individual-difference reliability. Where they are found to be reliable, researchers can then progress to discovering the variables that identify whether an individual will show an effect or not.** More specifically, even if a researcher is unsure about which individual-difference variables are going to be the best predictors of an experimental effect, a useful starting point would be to simply administer the experiment in two different testing sessions. If the experimental effect has high reliability (i.e., strong correlation between individuals' magnitude of effect in time 1 and time 2), then this is informative that there *are* stable individual-differences in the experimental effect. This means that future research can fruitfully then be focussed on discovering which variables are able to account for this stable variance.

The distinct implications of reliability in the individual-differences versus experimental spheres means that research practices that facilitate good experimental research can be suboptimal in individual-differences research. In particular, repeated-measures designs are common in experimental research, in which a participant contributes data to every condition. In such designs, it is crucially important to counterbalance the order in which conditions are completed across participants so that order effects (e.g., practice or fatigue effects) cannot confound the effect of the experimental manipulation. In contrast, where the *individual* is a variable of interest, then assigning different participants to different running orders introduces another source of variance in addition to the individual. This has led previous researchers to conclude that counterbalancing tasks or blocks introduces a confound into individual-difference designs (Dale & Arnell, 2013). Since a confound is a *systematic* source of error variance across the levels of the independent variable (which in this case, is the individual-differences variable under study), we can see some instances where this would introduce a confound, and some where it would instead introduce *random* error variance, and therefore should not artificially create an effect, but by increasing noise could decrease the sensitivity of the design. For example, if the goal was to assess the test-retest reliability of a given experimental task, then essentially one is asking the question of whether the task reliably ranks individuals across time. If the task consists of two blocks, and if individual A is assigned to running order 1 that is used in test 1 and test 2, and this differs from the running order 2 to which individual B is assigned, then if there are any order effects, these would inflate the likelihood of the task reliably differentiating between individual A and B, but it would be due to the order effect rather than anything intrinsic to those individuals. Where designs have this issue, reliability could be examined separately for individuals assigned to each running order to decontaminate reliability from order effects.

In other instances, however, counterbalancing would introduce a source of random error variance rather than a confound. While this is still not ideal, it is less problematic. For example, consider a design that consists of a single administration of an experimental task that contains two conditions in different blocks, and researchers measure the correlation between performance on this test and a questionnaire individual-difference measure. In this case, if participants are randomly assigned to one of the two possible block orders for the experimental task, then there should be no systematic relationship between how they score on the questionnaire measure and block order assignment. Therefore, even if one of the running orders tends to produce a stronger magnitude of the experimental effect, then this would add random rather than systematic noise to detecting the relationship between the questionnaire measure and the experimental task. Researchers should therefore consider using a fixed running order in individual-difference experiments.

However, there are some situations where this is also not feasible, or would undermine another aspect of the design. This would be where the design is also seeking to answer a question about the relative magnitude of an effect in one condition versus another – such a comparison is, of course, susceptible to order effects in the absence of counterbalancing. For example, in recent work in our lab, we were interested in comparing the test-retest reliability of three widely used measures of attentional breadth, as well as the extent to which they correlated with one another. Here, if a fixed running order was used, then it may have inflated the test-retest reliability of measures assigned to particular parts of the running order (e.g., that which participants completed first). In order to make comparisons across the tasks, therefore, counterbalancing was required. Altogether, this leads us to: **Recommendation #6: researchers should consider using a fixed condition or block running order for individual-differences research, and only deviate from this where it is justified or required to answer an experimental or comparative part of the research question.**

The final couple of recommendations relate to the lens through which individual-differences data are considered and analysed. Broadly, there are two major ways that researchers can treat individual-differences data for the purposes of analysis: dichotomous or continuous. For example, in considering the relationship between attentional breadth and working-memory capacity, attentional breadth could be dichotomized such that individuals are classified as either “narrow” or “broad” according to whether they fall above or below a particular cut-off score on the measure of attentional breadth, and similarly working memory capacity could be dichotomized such that individuals are classified as either “high” or “low” according to whether they fall above or below a particular cut-off score on capacity. Conversely, both of these variables could be operationalised as continuous. For example, RT in a condition that demands a large breadth of attention (controlling for baseline RT) could be a continuous measure of attentional breadth, and similarly working-memory capacity could be OSPAN score. The case has been made extensively elsewhere, particularly elegantly by DeCoster, Iselin, and Gallucci (2009), that in almost all circumstances, treating the data as continuous was optimal. Here, therefore, we echo this recommendation: **Recommendation #7: to use continuous measures of variables by default wherever practically possible in individual-differences research with cognitive measures, and only deviate from this where it is justified or required.**

The final recommendation relates to an emerging area of research, which while potentially relevant to both experimental and individual-differences research, has been found to be particularly useful for the latter. That is, the traditional mode of data-analysis in attentional research is to consider a measure of central tendency (typically mean) level of performance across a number of trials that constitute an experimental condition. However, this can obscure another very important source of information: *variability* across trials. This is exemplified analysis of data from the dot-probe task, in which participants are presented with two stimuli concurrently in different spatial locations, one of which has greater emotional significance than the other. The stimuli disappear to reveal a probe stimulus (e.g., dot, letter, etc) for which some type of perceptual judgement is required (e.g., detection, identification, localisation). Participants' RTs are compared for congruent trials (where the probe appears behind the emotionally-significant image) compared with RTs for incongruent trials (where the probe appears behind the emotionally-neutral image). If RTs are faster on congruent versus incongruent trials, then attention is said to have been captured by the emotionally-significant image (Bar-Haim et al., 2007; MacLeod et al., 1986). The dot-probe task has proved highly popular, with a meta-analysis confirming that anxious individuals show a reliable bias toward threatening stimuli on the measure (Bar-Haim et al., 2007). However, it is well understood by researchers familiar with the experimental tool that effects can be unreliable. Recent research has capitalised on this unreliability, not as a flaw, but instead as a reflection of attentional processes which are intrinsically dynamic. Therefore, it has been suggested that a more appropriate way to analyse dot-probe data is to examine the attentional bias over much smaller units of time (e.g., a handful of trials) than a whole experimental block of trials, and to compute measures of *variability* in the bias over time (Zvielli, Bernstein, & Koster, 2015). Indeed, recent research has suggested that this metric has much greater diagnostic validity than traditional approaches which consider only mean level of performance (Bardeen, Daniel, Hinnant, & Orcutt, 2017; Cox, Christensen, & Goodhew, 2017; Gladwin, 2017; Iacoviello et al., 2014; Naim et al., 2015; Schäfer et al., 2016; Swick & Ashley, 2017; Zvielli et al., 2015), particularly when considered in concert with contextual factors (Cox et al., 2017). That is, Cox et al. (2017) found that high trait-anxious individuals had higher variability scores than low-trait anxious individuals, only in a context where they were continually being exposed to threatening information. Traditional mean bias score did not reveal this effect. We predict that this approach of considering variability as a metric in its own right will have value well beyond the dot-probe literature.

As another example of the utility of quantifying variability, consider a scenario in which a researcher seeks to establish whether performance on the Stroop task is predictive of an individual's hypothetical *attentional control quotient* (ACQ), which is typically measured by a battery of other tests. If the Stroop task can predict this, then it could provide a quick estimate of an individual's ACQ without requiring the lengthy battery of tests. Since attentional control as a theoretical construct encapsulates the ability to regulate attention over sustained demands and minimise volatility in performance, the researcher may well find that analysing the magnitude of the Stroop effect in small chunks of trials (e.g., 10 trials), and then computing the variance of these effects across a whole block of trials (e.g., 200 trials, or 20 x 10 blocks) is a stronger predictor of ACQ than just an individual's mean Stroop effect averaged across the whole 200 trials. Therefore, **Recommendation #8 is that researchers consider variability in performance as a source of information in addition to mean levels of performance in individual-differences research where appropriate.**

In conclusion, integrating experimental and individual-differences frameworks has much to offer scientists seeking to elucidate a complete science of human psychology. However, individual differences research has unique considerations and challenges relative to traditional experimental research. There is an emerging awareness of these issues, with recent work highlighting the mathematical and statistical reliability issues across diverse areas. Here, we have discussed and synthesised these issues to arrive at a practical guide for researchers. While our discussions were contextualised in relation to visual attention, the conclusions and recommendations apply more broadly across psychology to inform and inspire future research in this fertile ground.

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